ANALYSIS

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SOME MODES OF SPEECH OF EMPIRICAL SCIENCE

By B. von Juhos

ON this occasion I wish to make some remarks about several modes of speech of the empirical science of to-day. Mr. C. G. Hempel has outlined and defended the theoretical points of view of Carnap and Neurath in several essays which have appeared in this periodical.[1] I myself have once already criticised[2] his formulations in this place, and Mr. Hempel in *Analysis* 3/3 recently tried to reject my arguments.[3]

We have to deal with the question, whether some modes of expression which are sufficient for the demands and proposals of the theory of science which Carnap and Neurath stand for, are compatible with certain fundamental modes of speech of empirical science, or of empiricism respectively. I have denied that they are in my essay mentioned above, and have given the reasons for my view with regard to different cases. Mr. Hempel enumerates three main points (l.c. p. 33), against which my arguments are chiefly directed : (I), (*a–d*) The " behavioristic " interpretation of psychological statements such as " I see blue " ; (II), (*e–f*) the view that in our empirical science even statements of the kind just mentioned might be " altered " or " abandoned " under certain conditions ; (III), (*g*) The proposal to express " epistemological " considerations in the formal mode of speech.

Perhaps Dr. Hempel does not know that it has become a habit of the " Vienna Circle " to designate the entire theory of science of Neurath and Carnap as " Physicalism". Otherwise he would not remonstrate with me (p. 33–34) saying that (I) only is connected with physicalism, whereas (II) and (III) are independent of it. It may be right, that only the conception according to

[1] C. G. Hempel, *On the logical positivists'* etc., *Analysis* 2, 49 ; *Some remarks on facts and propositions, Analysis*, 2, 93.
[2] B. v. Juhos, *Empiricism and Physicalism, Analysis* 2, 81.
[3] C. G. Hempel, *Some remarks on empiricism, Analysis* 3, 33.

which all meaningful scientific statements must be translatable into the language of physics, deserves the name of " thesis of physicalism." Neurath anyway has designated[4] his entire scientific-logical views, to which also (II) and (III) belong, as " Physicalism ", and I, too, spoke about " Physicalism " in this general sense. (So did Th. Vogel in *Erkenntnis*, 4/2, and I in *Erkenntnis* 4/6.) But Hempel can by no means deny, that the points criticised by me touch the scientific-logical views of Carnap and Neurath.

(I) (*a*). Mr. Hempel agrees with me (p. 35), that for a physicalist the spoken indications of a person, such as " I feel pain " possess the same epistemological character as the indications for instance of a barometer. With this Dr. Hempel has agreed to a position from which results the incompatibility of certain physicalist modes of expression with some modes of speech of empirical science. For either the physicalist must deny the character of a statement to a spoken indication as " I feel pain " as well as to the indications of a barometer. But this would be contradictory to the empiricist point of view, for which " I feel pain " may be true or false. Or, the physicalist must decide to describe also the indications of a barometer as true or false statements, which may be errors or lies, as these indications, according to Dr. Hempel are meant to have the same character as spoken indications of a person. This also would be incompatible with the mode of expression of empirical science. If Mr. Hempel removes this second consequence as being contrary to the idea of behaviorism (p. 35) he may be right in doing so, but these are the difficulties that result from accepting the starting-point of physicalism mentioned above. At any rate Mr. Hempel does not indicate how we could possibly avoid this dilemma, in which the physicalist theory of science in opposition to the empiricist mode of speech actually finds itself.

(*b*) If we alter an empirical statement, we may do this because we have made a mistake concerning it, or possibly because we have intentionally told an untruth. If we exclude for a statement the possibility of its being an error, in case it is false, this exclusion belongs to the rules according to which the respective statement is to be used. Considerations referring to this, undoubtedly belong to the logical syntax of a language, consequently, if the language of science is concerned, to the logic of

[4] O. Neurath, *Radikaler Physikalismus und Realismus, Erkenntnis* 4/5.

science. Although the physicalists Neurath and Carnap give us full details of the circumstances which lead to the alteration of empirical statements, Mr. Hempel feels obliged to object, that my remarks about error and lie have not the slightest connection with the formulations of the physicalists (p. 36–37). The reason he gives is, that nothing is ever said in the papers of the above mentioned physicalists concerning error and lie. Now this is not quite right, for Carnap does make an allegation to that point in *Erkenntnis* 3, 218–220. But even if nothing were mentioned about it in the papers of the physicalists, their syntactical considerations may lead to consequences, which are decisive for the question of " error-lie." And to conceive this merely logical connection is really not so very difficult.

The physicalists Carnap and Neurath declare all empirical statements to be hypotheses. Besides, according to their opinion, such statements may always be tested only by spatio-temporal data, consequently only state spatio-temporal events. Statements such as " I feel pain " therefore only indicate physical reactions of the person who is making them. Now the language of present-day psychology makes the syntactical decision that, if somebody asserts " I feel pain " this statement can not thereafter be designated as an error. Mr. Hempel agrees, that it would be " very little expedient " (p. 37) that psychology should alter the rules for using its words and statements so that such a statement as " I feel pain " could be designated as an error ; but he emphasizes, that in principle the possibility of such an alteration does exist (p. 36), a fact which I have never denied. Only the meaning of the respective signs is altered also thereby.

Now I continue: if the above mentioned linguistic rule of psychology is compared with the syntactical rules proposed by the physicalists, their incompatibility becomes obvious. If a person asserts " I feel pain ", a hypothesis is established according to the physicalists. The rules according to which a hypothesis is used in empirical science determine that a hypothetical statement may always turn out to be an error, and as such may be altered. But in the case mentioned before, this contradicts the stated syntactical determinations of the language of psychology, following which a statement such as " I feel pain ", as soon as made is not allowed to be altered as an error (but at most as a lie). That a coincidence may, of course, be established by alteration of the linguistic rules either of the pyhsicalists or of

empirical science (the alteration of the rules of a revisional process belongs here too), as Dr. Hempel asserts on p. 36 and 37, is trivial. Only we must bear in mind, that thereby the statements, the practical rules of which are changed, also suffer an alteration of their meaning. So I must state again that Dr. Hempel does not refute my assertion that the linguistic-logical rules proposed by the physicalists lead to a contradiction of certain modes of expression in present-day psychology. And I find it incomprehensible how Dr. Hempel, in the face of these simple and clear relations, can assert (p. 36, 37 and 39) that my remarks about lie and error have no connection with the scientific, theoretical considerations of the physicalists Carnap and Neurath. This is the more astonishing, as since then Prof. Schlick,[5] also in a disquisition on Dr. Hempel's theories about the physicalists, in a paper in which he starts from the same point as I did, has reached similar results and asks similar questions concerning error and lie.

By means of several examples Dr. Hempel tries to demonstrate (p. 36), that psychology, under certain circumstances allows the mode of expression that we are mistaken concerning statements, the difference of which from such statements as " I feel pain " is only one of degree. So the difference between the statements " I change my political party out of conviction ; I am sure that I have not got the least desire for material advantages " and " I feel pain " is, as he says, only one of degree, and for all that a psycho-analyst might possibly prove that the man, who has made the above mentioned statement, was mistaken. I must object to Dr. Hempel that he quietly alters the meaning of the statement, that is to be tested. For even if some " motives ", that influence the actions of the man concerned are perhaps unconscious or subconscious on his part, he may quietly answer the psycho-analyst : " My statement does not say anything concerning such ' motives '. By ' convictions ' and ' motives ' I understand experiences, known to me directly, as for instance a headache, and nothing besides." The psycho-analyst by considering subconscious motives, no doubt understands by " motive " something different from our man, and therefore the statement tested by the psycho-analyst designates different facts from the statement made by the other. The statement of the psycho-analyst already contains *hypothetical* elements and

[5] M. Schlick, *Sur le Fondement de la connaissance*, Paris 1935 (Three essays, pp. 44–54).

therefore is different in principle, not only in degree from statements such as " I feel pain ". Therewith Dr. Hempel's argument becomes untenable.

Of statements of the latter kind I have asserted " while formulating a proposition such as ' I feel pain ', I know already whether I have made a true or a false statement." To this Dr. Hempel remarks : " I think, that with equal justice, one could say : ' When formulating a statement about the real motives of my action, I know already if I am right or wrong'." (p. 36 note 9.) It is by no means necessary to be very informed about the logic of language to see that, contrary to Dr. Hempel's opinion, we have to deal here with two completely different cases. Of course, if a person asserts " I do this because it pleases me " I naturally assume that a statement similar to the statement " I feel pain " is referred to, and I also assert that the man concerned must know at the moment when he is making the statement whether he has formulated a true or a false statement.[6] But when Dr. Hempel asserts quite generally that any not precisely defined statements made by me about the motives of my actions are of the same kind as " I feel pain ", this is surely an untenable assertion. Neither can the objection of Dr. Hempel (p. 36, note 9) stand the test against my view, that in a contradictory protocol-statement the person who has made the statement cannot have made a mistake, but *must* have known that he has written down a false (or better, meaningless) thing. Mr. Hempel objects to this that he, when teaching his pupils mathematics, has had contrary experiences. It only can lead to confusion, if a discussion about empirical statements glides into one about mathematical statements unawares. But, apart from that, it escapes Dr. Hempel's notice, that his pupils wrote down contradictory mathematical statements, because they were not sufficiently acquainted with the rules, according to which mathematical symbols are used, in other words, because they had not yet mastered, i.e. understood, the mathematical language. But what I have assumed (*Analysis* 2, 90), was exactly that the protocolling person fully understands the language in which he is making the protocol, that he is fully acquainted with the rules

[6] A. J. Ayer (*Language, Truth and Logic*, London, 1936, p. 125–135) tries to prove it logically impossible that empirical statements exist that are undubitable. In his opinion it is logically inadmissible, i.e. leading at any rate to a contradiction, to establish such rules for an empirical statement that its alterability becomes impossible. Ayer thus brings himself into opposition not only to my conception, but to that of Carnap and Neurath as well.

concerning the use of words and sentences. Dr. Hempel's objection evidently does not affect this case. I still assert that he who knows exactly the rules of the language in which he expresses himself must also know, when protocolling a contradiction, that he is violating the rules of his language and therefore intentionally making a false assertion.

(*c*) Mr. Hempel expresses (p. 37) the behaviorist thesis of the physicalists, as I believe, at least incompletely. For he says physicalism is satisfied with stating that in everyday-life, as well as in psychology, all methods of testing statements about sentiments, thoughts etc., consist in examining a person's behaviour. As far as I know the physicalists assert considerably more.[7] According to them, a psychological statement is nothing but an assertion about spatio-temporal events, i.e. it must always be translatable into a statement of this kind. If such a translation is impossible, if no physical data of the kind asked for can be given, then the statement in question according to the physicalists is not a meaningful statement at all, but a meaningless succession of words. "I feel pain" consequently is for the physicalist merely an assertion about the "behaviorist" data concerned. Suppose a man has no idea of the psychological meaning of these physical data, but is complaining of pain. He surely does not mean by his assertion any "behaviorist" symptoms in his body or nerves. He would answer all such questions as: "Do you mean by your assertion that you are crying, moaning, writhing etc.?" with "No, I mean something else." Did he, therefore, not mean anything meaningful by his assertion? I should say yes, he did, though! A consistent physicalist, according to my opinion, would have to contradict me. This result I have also reached in my earlier essay (*Analysis*, 2, 85). Unfortunately Mr. Hempel's reply (p. 37) which, as I said before, does not completely reproduce the physicalist conception in this point, does not clear up the question how we could escape this absurd consequence, and at the same time retain the physicalist position.

(*d*) My example, by which I tried to show that it is conceivable that such statements as "I feel pain" might very well be controlled by strangers in a non-behaviorist way (*Analysis* 2, 88–89),

[7] R. Carnap: *Die physikalische Sprache*, etc., *Erkenntnis*, 2, 432; *Psychologie in physikalischer Sprache*, *Erkenntnis* 3, 107; *Erwiderung*, etc., *Erkenntnis* 3, 177.
O. Neurath: *Radikaler Physikalismus*, etc., *Erkenntnis*, 4/5.

seems to have been misunderstood by Mr. Hempel. He thinks (p. 37 and 38), that the blind man B, who has always got the colour-experience " blue " when the man S, who sees, has got it, could control the statement of S " I see blue " only by establishing a general law, which expresses the simultaneousness of their colour-experiences. B can establish such a law only by noticing the spoken indications of S (no other " behaviorist " data are to be accessible to B). This interpretation of Dr. Hempel is not right. It never comes to an establishment of a general law. If I control my own statement " I see blue ", I do not do this with the help of a general law, which asserts that a corresponding colour-experience always arises simultaneously with my spoken indication. Even if I learn the right use of the statement, partly by observing my behaviour, I yet control my statement, without the introduction of a hypothesis, simply by stating whether I have a certain colour-experience or not. And the same is the case with B. Though he must naturally observe the spoken indications of S, in order to learn the rules according to which S uses the statement " I see blue ", he does not, for all that, establish a general law in order to control this statement. Moreover, if the syntactical rules have become clear to him, the fact that he (B) has or has not had a corresponding colour-experience by itself serves him as a truth-criterion of the statement made by S " I see blue ". The control therefore takes place in a non-behaviorist way without the introduction of a hypothesis.

II (*e*) Dr. Hempel asserts (p. 38) that it is not proved that we, according to the physicalists' point of view, " . . . are allowed to alter the propositions obtained by observation as it pleases us ", as I had written. I should like to ask Mr. Hempel to look up in this connection *Logik der Forschung* by K. Popper (Vienna 1935, p. 54 and 55), where Mr. Popper repudiates the conceptions of Neurath and Carnap, because they do not indicate a method by which empirical statements may be altered, whereby " they throw overboard empiricism ". Where no precise method of the alterability is stated, we may use it as we please. It is not a mere chance that Mr. Popper in his criticism of the physicalists comes to the same result as I do, viz. that they are breaking with the principle of empiricism. Mr. Popper himself indicates a detailed method for the alterability of the empirical statements, and so makes a principle of drawing a line of separation between his own conception and that of Carnap and Neurath. My criticism

deals only with the latter. Mr. Hempel in order to refute my criticism, refers also to a paper of Neurath's,[8] which was published only after my essay had appeared in *Analysis*. There Neurath does concede to his adversaries that observation-statements possess a greater stability than other propositions, but he does not give us a method here either for restricting alterability at will.

(*f*) Mr. Hempel reproaches me with a contradiction (p. 38) as I, on the one hand, declare statements of the kind " I feel pain " to be unalterable, and on the other hand I concede that such statements may be lies, consequently may be declared false and may be struck out. Here I must retort that he overlooks an important passage in my argument, and that it is only in this way that he is able to construct his contradiction. In my essay I distinctly emphasize (*Analysis* 2, 89–90), that the " alteration " of an untruth told with intention is different in principle from the alteration of an error. It is trivial to say that having intentionally formulated a false statement (for instance a lie), I may declare it false thereafter and strike it out. On the other hand it is *not* trivial (and here the epistemological difference appears) when I assert a statement to be true, but see later, that I was mistaken, and have to alter the statement for this reason. This passage in my essay runs : " And if we have declared certain statements as unalterable, we did not mean anything but that these statements can never be errors and can therefore not be altered like errors." (*Analysis* 2, 90). If Dr. Hempel had taken this passage into consideration, it would have been impossible for him to construct the above mentioned contradiction.

Further on Mr. Hempel quotes my conception incorrectly (p. 39) ; there he says : " the adherents of the alterability of any scientific statement do *not* pretend "—as I believe mistakenly— " that the alteration of the considered psychological statements must always be carried out by assuming an error." This I never asserted ; but only that the linguistic rules proposed by the physicalists permit the alteration of the above mentioned psychological statements under the supposition of an error, whereas the modes of speech of present-day psychology exclude this possibility. The assertion of Dr. Hempel's that the distinction between lie and error has no connection with these questions (p. 39), I can only explain by the fact that the connections existing

[8] O. Neurath : *Pseudorationalismus der Falsifikation, Erkenntnis*, 5, 353.

here (see (*b*)) are not yet known to him. As has been said before, he will find this question dealt with also in the paper of Mr. Schlick, referred to above.

(III) (*g*) Finally Dr. Hempel repudiates (p. 39) my assertion that the formal mode of speech could not be employed in the discussion concerning which of the numerous imaginable systems of science which are free from contradictions is our genuine " true " science. He admits, that he has employed the material mode of speech in his first paper (*Analysis* 2, 57), when characterising the " true " science, but he believes that I have not translated the respective statements adequately into the formal mode of speech and that I was only thus enabled to draw the conclusion that, with the help of the criteria stated by him, an unlimited number of systems of science which exclude each other might be designated as equally true. Dr. Hempel himself gives (p. 39) a translation of the decisive statement into the formal mode of speech : firstly he declares this translation to be correct and secondly asserts that the difficulties pointed out by me cannot be deduced from it. According to him the correct formal translation runs as follows : " The following statement is sufficiently confirmed by the protocol-statements adopted *in our science* : ' Amongst the numerous imaginable consistent sets of protocol-statements, there is in practice exactly one which is adopted by the vast majority of instructed scientific observers; at the same same time, it is just this set which we generally call true.' " We shall name this complicated sentence P.

Is it really true that by P, among the innumerable systems equally possible, only one, viz. our empirical science, is characterised as " true " ? Let us imagine a number of sets of protocol-statements free from contradiction; the different sets are meant to exclude each other, but they must not contain statements which assert the adoption or the repudiation of the set concerned by the vast majority of our scientists. An infinite number of such sets can be constructed. To each of the sets we add protocol-statements, which indicate that in practice the respective set of protocol-statements is the only one of all possible sets which is adopted and called true by our scientists. The single sets of protocol-statements thus remain free from contradiction. A scientific system based on such a set of protocol-statements is then already to be called " our science ". Corresponding to the numberless possible sets of protocol-statements of the kind

described, there are infinitely many systems of science, that exclude each other and are free from contradiction. They may be designated by S_1, S_2, S_3, . . . all of which are to be called " our science ". And the protocol-statements which confirm an S_n, firstly are considered as adopted by " our science " and secondly confirm the following statement : " Amongst the numerous imaginable consistent sets of protocol-statements, there is in practice exactly one (viz. the set of protocol-statements which confirms S_n) which is adopted by the vast majority of instructed scientific observers ; at the same time it is just this set which we generally call true." But as there are, as we said before, infinitely many S_n's, which all satisfy Dr. Hempel's conditions, there are also infinitely many " true " sciences. The new formulation of Dr. Hempel's therefore leads to the same difficulties, that I (*Analysis* 2, 91–92) deduced from his original mode of expression.

Perhaps Mr. Hempel believes he can escape these difficulties by italicising in his statement P the words " in our science ". Does he, perhaps, wish to express by this that only those protocol-statements pass as " true ", which are recognised by our science, *whose existence simply is a fact*? To be sure, this set of protocol-statements would then really be distinguished among all other possible ones, as the only true one, only this would have been expressed in the *material* mode of speech. (Other logical objections against the statement P we do not want to discuss here.) If Dr. Hempel imputes this sense to his italicised words, he is mistaken when he claims to have made the statement P in the formal mode of speech.

Finally Dr. Hempel emphasizes (p. 40) that it is also shown here that the formal mode of speech is preferable, because it excludes such misunderstandings as the difficulties which are discussed by me in *Analysis* 2, 91–92. If the mode of speech employed by Mr. Hempel is, as he asserts, the purely formal one, then I believe I have demonstrated that he does, for all that, *not* escape the difficulties mentioned above.

Vienna, April 1936.

ANALYSIS, ANALYTIC PROPOSITIONS, AND REAL DEFINITIONS

By DONALD C. WILLIAMS

EVERY word—except logically proper names, if such exist—is used to connote a certain character, simple or complex, which may be called " the defining character " of the word. It denotes the instances of that character.

A sentence whose subject-phrase connotes a complex character or conjunction of characters, $\phi\psi$, and whose predicate-phrase connotes a constituent or factor of that conjunction, ψ, is an *analytic* sentence, that is, a sentence which expresses an analytic proposition. An analytic sentence like " Red apples are red " is uninteresting because the factor named by the predicate is named singly by the same word in the subject-phrase. " Apples are fruit " is more interesting because the word " apples " connotes by itself a conjunction in which the character connoted by " fruit " is a factor. I shall draw my examples from the latter sort of analytic sentences, but the two sorts do not differ logically. An analytic sentence is tautological. It is true[1] *a priori* and necessarily. A sentence whose predicate connotes a character which is not a factor in the character connoted by the subject, is *synthetic*. If it is true, it is true *a posteriori* and contingently: that is, the character connoted by the subject is empirically associated with the logically independent character connoted by the predicate.

A definition is a statement that the word which it defines is or shall be used to connote a certain character.[2] In this strict sense, a definition is by definition a nominal definition. It is a statement about a word. The word as it occurs as the definiendum does not connote its usual defining character, say $\phi\psi$, but connotes simply the character of being that kind of a word, say κ, and the definiens connotes the character of usually connoting the character $\phi\psi$. If the definition is to be of any practical value, the definiens must name the character, $\psi\phi$ by

[1] I think it appropriate to speak of the truth of a *sentence*. Persons who think otherwise can interpret the phrase as an ellipsis for the truth of the corresponding " proposition." I try consistently to use " connote " for the relation by which any word—noun, verb, or adjective—signifies a character. The notion that " names of simple qualities " are non-connotative (Stebbing, *A Modern Introduction to Logic*, p. 32) is, I think, an error with extremely unfortunate results.

[2] By a usage equally legitimate, the word " definition " means, not the *statement* that the word is used in such and such a way, but the actual *fact* that it is so used. It will be clear from the context when I am forced to use " definition " and " define " in the latter sense.

words other than the definiendum. *A definition is always a synthetic statement.* A definition is true if it correctly describes or predicts the use of the definiendum, false if it does not.

There is a persistent logical tradition that some definitions are not merely descriptions of the use of a word, but are somehow "about *things*." These are called "real definitions." It is now widely conceded that the word "definition" ought to be reserved for nominal definitions, and several attempts have been made to explain, or explain away, the traditional notion of real definition.

The phrase "real definition" has covered some doctrines which are quite futile and unintelligible: the notion, for instance, that there are definitions which coincide with an intrinsic or sympathetic connection between word and thing. Such a notion was betrayed by Mark Twain's Eve, who named the hippopotamus "hippopotamus" because it *looked like* a hippopotamus, and by the man who marvelled at the astronomers' ability to discover the names of the stars. It has probably been held by more solemn logicians than one might suppose.

Other notions intended by the phrase "real definition" are those of special sorts of nominal definitions. These are intelligible and workable, but comparatively trivial. They include such notions as those of: (*a*) (nominal) definitions which are *true*; (*b*) definitions which describe standard social usage rather than a peculiar private terminology; (*c*) definitions which have actual existential application, i.e., impute characters which have instances; (*d*) definitions which impute characters which are important or crucial, corresponding to real kinds, dynamic units, natural lines of cleavage. But I suspect that there has been more in the doctrine of real definition than is contained in any of the foregoing concepts.

The usual dichotomy of analytic and synthetic statements is not obviously exhaustive. There are words, like "unipara," which have been expressly defined to connote conjunctions of certain simpler characters—in this case, the complex character of being a female parent of one and only one child. The sentence "All uniparas are mothers" thus represents unequivocally an analytic proposition. What we mean by "unipara" expressly contains what we mean by "mother." On the other hand, "Americans are conservative" represents unequivocally a synthetic proposition, because the defining character of

"Americans," a purely geographical property, is in no sense a conjunction in which the character of conservatism is a factor or conjunct. But what of sentences like: "The sensation of heat is compounded of the sensations of warm, cold, and pain;" "Squares are rectangular;" "Piety is doing what is pleasing to the gods;" "The enjoyment of tragedy is a purging of pity and fear;" "Knowledge is true belief"?

The peculiarity of the third class of sentences is that although the person who enunciates them believes that the characters singly connoted by the predicate-words are genuinely constituents in the complex character connoted by the subject-word, they are not so in the same unmitigated sense as in the sentences which we called "unequivocally" analytic. Sentences of our third class are not plainly *a priori* and tautological, such that to understand them is to accept them, but are open to dispute, even among persons who agree on the connotations of the several words.

The root of this peculiarity seems to be the following. The subject-words of obviously analytic statements have been deliberately defined to connote a character-complex expressly identified by its several factors, while the predicate names some or all of these expressly recognized factors. But such a deliberate definition to connote a complex of a specified constitution is rare. It is confined chiefly to scientific neologisms, which are short-hand expressions, coined for convenience, to connote conjunctions which give promise of being dynamic units or real kinds. Most words which connote complex characters—including the subject-words of our third class of sentences—are defined undeliberately by popular usage to connote the characters *as wholes*, as *Gestaltqualitäten*, without express recognition of their constituent factors. It is a fundamental fact of human psychology—if nothing more—that the mind, or the organism, can respond differentially to such mass-effects, and name them consistently, without having responded abstractively to, and without naming, their constituents.

A statement of the third sort shares with analytic sentences the obvious property, that, if it is true, its predicate connotes characters which *in some sense* are factors in the character connoted by the subject. But it shares with synthetic statements the fact that in some sense the predicate-characters are not factors in the character connoted by the subject, for the reason that the subject-character is in some sense elemental. Like the synthetic statement, such a statement is apparently *a posteriori* and disputable.

The senses in which a whole gestalt-quality is and is not identical with its own structural constitution are quite perspicuous and primary facts of experience, but they are difficult of conceptual formulation. The problem which they set is no doubt cardinal not only to the general question of the nature and status of qualities, but to the question of the propriety and significance of analysis. The synthetic aspect of our third type of sentence is the logical correlate of the phenomenon of "emergence," and of the reputed metaphysical truths that a whole is more or other than the collocation of its constituents, and that the analysis of a familiar object can be said to "belie the object" or "issue in genuine discovery," according as you prefer to look at it.

I do not intend to pursue these problems here or to attempt to decide whether our third type of statements should be called "analytic" or "synthetic." I shall call them "resolutive."[3] It will be plain that resolutive statements are by no means rare, vague, and profitless. It is they, and not so-called analytic statements, which are chiefly sought by analytic science.

Each of our three classes of sentences or statements may be further divided into two sub-classes, one sub-class consisting of sentences which represent simple implications, strict or material as the case may be, and the other sub-class consisting of sentences which represent the corresponding reciprocal implications or equivalences. This division furnishes, so to speak, a new table of predicables.

(Ia) "A unipara is a mother" is an analytic statement of (strict) implication. (Ib) "A unipara is a mother of only one child" is an analytic statement of (strict) equivalence. (IIa) "Man is full of trouble" is a synthetic statement of (material) implication. (IIb) "Man is the biped who can learn grammar" is a synthetic statement of (material) equivalence. (IIIa) "Knowledge is belief" is a resolutive statement of (what kind of?) implication. (IIIb) "Knowledge is true belief" is a resolutive statement of the corresponding kind of equivalence.

The words "real definition" may have occasionally been used to mean plainly analytic statements of intensional or strict equivalence (Ib). ("A unipara is a mother of only one child" must be distinguished from "'Unipara' connotes the character of being the mother of only one child." The latter is a definition in our sense, a nominal definition, and is a synthetic statement

[3] I suppose that this word was suggested by W. E. Johnson's *Logic*, I, p 111, but it does not here mark the same distinction which it marked for him.

about the word " unipara." The former is not a definition in our sense, but an analytic statement about the nature of uniparas.) " Real definitions " may occasionally have been used, also, to mean plainly synthetic statements of material equivalence (IIb). But I suspect that the most profitable and significant use of " real definition " has been to mean resolutive statements of equivalence (IIIb) : that is, statements of genuine and fallible analysis. This may be what is meant by the phrase " a real definition describes the real nature of the thing," or " a real definition gives an analyzed equivalent of the original meaning." So far as I recall, Cohen and Nagel[4] and the only logicians expressly to describe this function of real definitions. If this is what a real definition is, then real definitions are very important components of discourse, but they ought not to be called " definitions."

Verbal usage gradually changes in both common and scientific speech, according as our estimate changes of what character-complexes are real kinds or dynamic units. Sometimes new words are coined, either for an unanalyzed gestalt-quality or (especially in science) for an express conjunction. Very often old words are re-employed for a new connotation, usually to connote a character which was a factor in the old connotation or for a character which has been empirically associated with the latter. Such process results, of course, in once analytic sentences becoming synthetic, and conversely. But the closest of all associations is that between a gestalt-quality and the conjunction of its putative constituents. Sometimes a word, like " automobile," originally expressly defined to mean a conjunction of discriminated characters, comes to be used for the gestalt-quality equivalent to that conjunction. When this happens, statements once clearly analytic (" Automobiles are mechanically propelled ") become resolutive. On the other hand, it may happen that a word, like " velocity," which once connoted a gestalt-quality, may come to be used expressly for the conjunction of its putative constituents. When this happens, statements once resolutive become clearly analytic. This is probably, indeed, the only source of analytic statements other than deliberate neologism (and, of course, the kind of combination exemplified by " Red apples are red.") It is the source, I suspect, even of those analytic formulae which are the bases of pure logical systems.

The method of analysis, in science and philosophy, from the Socratic quest of definitions to the newest positivist, consists

[4] *An Introduction to Logic and Scientific Method*, p. 230.

primarily in the framing of resolutive statements of equivalence —that is, the framing of what have been called " real definitions." (The foregoing sentence is itself proffered as a real definition of *analysis*.) When the method is most successful, the conjunction of characters connoted expressly by the predicate of its statement is accepted, technically or even popularly, as the defining character of the subject-word. The formula is then no longer a " real definition " but a purely analytic statement of equivalence (or identity), and the statement that the subject-word of this statement connotes the conjunction connoted by the predicate words, is a definition in the strict sense of " definition "—a nominal definition.

Different persons, or the same person at different times, with varying sophistication, will use a word both for a gestalt-quality and for the conjunction of its supposed constituents. The sentence " A square is a rectangle " doubtless represents to the *Gelehrten* who read this journal a plain analytic proposition. To the commonality, however, it is resolutive. This fact makes it difficult to give unambiguous examples of our thesis, but provides an opportunity for its application.

The shift of meaning described above is an unmistakable change of attitude and intention. Its nature, however, is obscured by the fact that, until the shift has occurred, the subject-word connotes a character of which our knowledge, however *bona fide*, is necessarily inarticulate. A gestalt-quality has, as such, the epistemic status of a simple quality, and by hypothesis a word connoting it cannot be defined analytically, that is, by words severally connoting the several constituents of the defining character, but only ostensively or (in some cases) bi-verbally or by " translation." When a plausible resolutive equivalent is enunciated for the gestalt-quality, no previous definition is on hand to compete with it, and we are tempted therefore to say that this equivalent is " what we meant all along." That we did *not* mean it all along, even though we are going to mean it in the future, is proved, I think, by the facts that we previously had not, and probably could not have, enunciated such a resolutive statement, and that it can happen, and often does happen, that another analyst will give us for the same gestalt-quality another resolutive description which we can with equal plausibility declare to be " what we meant all along."

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